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Baum's Private Thoughts

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I find Baum's arguments in this article utterly convincing. That should not be surprising, because Baum and I think alike. It might be worth explaining why this is so. It has to do with our common history of reinforcement and punishment. We occupied desks in the same tiny cubicle in the basement of Harvard's Memorial Hall during the 3 years (1962–1965) it took us to get our PhDs. Together we suffered under the oppressive system operative there. We stood in awe of Skinner but were more directly influenced by Herrnstein and Stevens. To our many discussions and near-violent arguments, Baum brought his background in biology and I mine in mechanical engineering and gestalt psychology. These backgrounds gave us the confidence to focus on evolutionary, operational, and molar aspects of our teachers' ideas: Skinner's concept of the generic nature of stimulus and response and of the operant, Stevens' pragmatic insistence on simplicity in theory and experimental design and, most of all, Herrnstein's matching law, the molar implications of which we were just trying to comprehend.

Herrnstein had circulated a working paper applying matching to individual responses. His original account applied to symmetrical choices between Response A and Response B; now he applied the matching equation to a choice between Responses A and not-A (all behavior except Response A). According to Herrnstein (1970), the rate of any particular response was directly proportional to the rate of reinforcement of that response alone, and was indirectly proportional to all of the reinforcement in the situation (including the reinforcement of that response as well as the reinforcement of all other available behavior). Both Baum and I were bothered by this account. These extra reinforcers could not be measured directly. We both firmly believed in the minimization of free parameters (we still do), and Herrnstein had just added another free parameter to the matching equation. Also, Herrnstein's new formulation made a prediction that went against our expectations. Suppose a pigeon's pecks were being reinforced on a variable-interval schedule (the pigeon consequently pecking at a steady rate) and then free reinforcers, independent of pecking, were randomly given to the pigeon (in addition to those it earned on the variable-interval schedule). It seemed to us that the extra reinforcers, occasionally occurring within a short time of a peck, might superstitiously reinforce pecking and therefore increase pecking rate. The higher the rate of these free reinforcers, the more accidental contiguities would occur between pecks and reinforcers, the faster the pigeons should peck. However, Herrnstein's new formula said that pecking rate would vary *inversely* with the rate of these free reinforcers; the faster they came, the slower the pigeon should peck. I recall the three of us sitting in Herrnstein's office as we presented this contradiction to him. But he was

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unfazed. He was sure that under those conditions response rate would decrease; he encouraged us to do the experiment, which we did, and, of course, he was right. The pigeons' behavior was controlled not by discrete, superstitious contiguities between response and reinforcer but, as Herrnstein's formula predicted, by the correlation between response rate and reinforcer rate over the duration of the session (Rachlin & Baum, 1972). The randomly scheduled free reinforcers reduced this correlation and thus decreased pecking rate. The more free reinforcers, the slower the pigeons pecked. Moreover, the mathematical form of this rate reduction was exactly that predicted by Herrnstein's formula.

Later, Herrnstein, together with Philip Hineline, our fellow graduate student and office mate (one cubicle over), published their groundbreaking studies of avoidance (Herrnstein & Hineline, 1966) in which rats could learn to press a lever to avoid shock only when overall rate of shock varied inversely with overall rate of lever pressing (regardless of contiguities, accidental or imposed, between individual responses and individual shocks). These experiments made us dedicated molarists. In a sense, all of our work since then has been an extension of these original ideas.

I believe that, had behaviorism retained the respect of philosophers over the years since then, neither of us would have attempted to draw out the wider implications of this point of view. But, because behaviorism has been rejected by philosophers for inadequately explaining mental life (Block, 1981), it seemed to us important to examine whether the philosophers' refutations applied to our molar behaviorism. It seemed and still seems to us that the arguments of the philosophers are not valid against our molar behaviorism, a behaviorism that views mental life in terms of the interaction over time between the environment and the organism as a whole. As Baum's article shows, the situations and imaginative constructions of the philosophers are actually better explained by our molar form of behaviorism than by the neurocognitive model so much in vogue today.

Let me underline this point by revisiting one of the examples in Baum's article, Dennett's example of Tom who is riding the bus because (a) he wants to go home and (b) he believes that the bus will take him there. Philosophers believe that behaviorists cannot explain such behavior because Tom is only doing one thing yet he is doing it as a consequence of the interaction of two reasons (his knowledge and his belief). There seems to be no reinforcer of his single act that can capture the dual mental states, the interaction of which apparently causes the act. But let us suppose that knowledge and belief are not private entities interacting like two gears in a person's head but are two temporally extended patterns of overt behavior shaped, like any patterns of overt behavior, by contingencies of reinforcement. For example, for the past 4 years I have been swimming about four times per week and, each time, I swim 30 laps of the pool. That comes to about 120 laps per week. This is a purely behavioral fact about me. Just as clearly, at every moment during the month, including right now while I am sitting here typing on my computer, I am swimming at the rate of 120 laps per month. Similarly, I am also sleeping about 7 hr per night. My rates of swimming and sleeping are two overlapping long-term patterns of my behavior. My swimming rate consists of both time swimming and time not swimming. My sleeping rate consists of both time sleeping and time not sleeping. What I am doing right now (neither sleeping nor swimming) belongs to both patterns simultaneously. This is obvious and hardly worth pointing out. The obviousness is due to the fact that

swimming and sleeping are both clearly overt behaviors. If our mental vocabulary referred to similarly overt patterns of behavior, that is, if believing were one pattern of overt behavior and desiring were another pattern, as Baum and I believe, then Tom's current bus riding would just as clearly be seen as a single short-term act belonging to both his desire to get home and his belief that the bus will get him there.[1] Thus Dennett's problem is not a problem for a molar behaviorism, but it is a problem for his own neurocognitive theory in which mental states are essentially private events that supposedly can be efficient causes of overt actions.

An antiprivacy machine (Figure 1 of Baum's article) could never work. The reason is that our thoughts are at their clearest to ourselves, as well as to others, when they are observed as overt patterns in our behavior. The thought, "Who am I?" even if never verbally expressed, is clearest in the thinker's patterns of behavior over time. Suppose she actually said out loud the words, "Who am I?" Would that speech be her thought? No, it would just be a small piece of her thought. To know her complete thought we would have to know *why* she said those particular words: her goals and the part saying those words played in achieving those goals. Now suppose she did not say the words out loud but only to herself (as in the balloon in the figure). Where can we go now to find her thought? Suppose we could measure minute movements of her tongue as she repeated those words to herself. Her thought would be less clear and complete than it would be if she said the words aloud. Her thought would be even less clear and less complete as activity in her peripheral nerves; it would be still less clear and still less complete as an MRI record. As you went deeper and deeper into her neuroanatomy, her thought would become less and less clear and complete, until it finally faded away completely into the chemistry and physics of her brain. To look inside a person's head for her thoughts or for any aspect of her mental life is to go in precisely the wrong direction. The sentence she says to herself may be a *potential* thought. But her thoughts themselves can be clear, complete, and *actual* only in her overt behavior over time.

Baum and I agree on this. Do we agree on everything? Not entirely. Baum prefers to call himself a "molar behaviorist" while I prefer "teleological behaviorist" to emphasize the use of Aristotle's concept of final causes. One way to think of final causes would fit with Skinner's notion of reinforcers as causes of responses. In this sense, food delivery may cause a pigeon to peck a key. Another example: Suppose I buy a bat in order to play baseball. Playing baseball would be the final cause of buying the bat, the reason *why* I buy the bat.

But another way, a better way, of understanding the meaning of final causes sees the final cause as the whole pattern of behavior into which the individual act fits. In this conception, the pattern, pecking-plus-eating, would be the cause and *both* pecking and eating the effects. Or, I *swing* a bat in order to play baseball. Swinging a bat (unlike buying a bat) is itself part of playing the game. This latter view fits with the methods of behavioral economics in which a utility function is a final cause. In behavioral economics, a person's behavior is observed under one or two or three or more sets of constraints. The behavior is said to "reveal" the

[1] Aristotle (*De Anima*, Book 3, chap. 2, 426b, 22) confronts the same problem in his discussion of how we discriminate *white* from *sweet*. His solution is to consider it to be the behavioral overlap of an overt discrimination (over time) by an individual of white from nonwhite and a discrimination (over time) of sweet from nonsweet.

person's preferences. From this behavior a utility function is constructed under the assumption that utility is maximized by the behavior. Then the utility function is used to predict behavior under further sets of constraints. Constraints, for the economist, are contingencies for the behaviorist. Discount functions are utility functions. Herrnstein's original matching law and Baum's (1979) generalization of it may be interpreted in terms of utility functions (Rachlin, 1989). The phrase ''teleological behaviorism'' implies these relationships, and that is why I like it as a description of how both Baum and I think, that is, of the common patterns in our behavior.

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